The violence with which false interpretations were put upon this Theory and a function was assigned to it which it could never fulfil, will some day be recognised as one of the least creditable episodes in the history of science. With a curious perversity it was the weakest elements in the Theory which were seized upon as the most valuable, particularly the part assigned to blind chance in the occurrence of variations. This was valued not for its scientific truth,—for it could pretend to none,—but because of its assumed bearing upon another field of thought and the weapon it afforded for expelling Mind from among the causes of Evolution.

There have been many symptoms that this Philosophy is breaking down. Mr. Herbert Spencer, although he has worked out the consequences of Evolution with enthusiasm, has never been blind to some of its defects. His mind is too closely analytical not to be brought into contact at many points, with its manifest inapplicability and its wordy hollowness.

But in these two articles we have for the first time an avowed and definite declaration against some of the leading ideas on which the Mechanical Philosophy depends; and yet the caution, and almost the timidity, with which a man so eminent approaches the announcement of conclusions of the most self-evident truth—is a most curious proof of the Reign of Terror which had come to be established.

I cannot in this letter indicate the breadth and sweep of the admissions now made by Mr. Herbert Spencer in the two articles referred to,—fatal to the adequacy of the Mechanical Philosophy as any explanation of organic evolution. They cluster round, and follow from the central admission that "the words 'natural selection' do not express a cause in the physical sense." Another great admission is that the "co-operation" which is required in the growth and development of useful parts, cannot be accidental.

Of course, now that so eminent a man as Mr. Herbert Spencer has opened his eyes and his mouth to these—and many other—admissions, we shall have all the *Dii Minores* following suit.

I have read with great pleasure an article in your last number (p. 314) on "Physiological Selection," with an "additional suggestion on the origin of species." I rejoice that the author has at last discovered that "natural selection has been made to pose as a theory of the origin of species, whereas in point of fact it is nothing of the kind." This has been my contention for many years.

ARGYLL

Aurora

WITH reference to the aurora of July 27, accounts of which appear in NATURE, vol. xxxiv. pp. 311 and 312, the following particulars of the accompanying magnetic disturbance recorded here may be of interest. The disturbance commenced about 3 p.m. on July 27 with small fluctuations in declination and horizontal force, followed by larger movements which commenced sharply at 10.20 p.m. in all three elements, and continued to about 7 a.m. on July 28. The greatest movement was between 10.20 and 11.30 p.m., amounting to 45′ in declination, ·011 of the horizontal force, and ·005 of the vertical force. Corresponding earth-currents were recorded as usual.

W. H. M. CHRISTIE

Royal Observatory, Greenwich, August 10

Mock Suns

KINDLY add the following, to make up for omission of my figure in your issue of July 29 (p. 289):—

The "arched eyebrows," as I called them, can best be described thus—

Resting on the top of the halo circle, where the third mock light stood, was a bow of peculiar curve. It was like two well-arched eyebrows flowing together by a curve of gentle dip at the point where it touched the halo. Each arch was about equal to one-eighth part of the halo circle in every respect except that its centre lay about the middle of the chord joining the upper mock light with the mock light on that side of the sun. The contrariflexure, and the anomalous positions of the two centres of the two arches, strike me as very noteworthy. I cannot presume to guess at an explanation.

May I add that a correspondent of the *Standard* states that he too saw the white ray on the *left* side; and that it stretched, to use his expression, "round the sky almost to the east, and at the end of it was another mock sun much less brilliant," where it "seemed about to begin a fresh series of mock suns and circles." This too seems to me too striking a feature to be lost to record in NATURE.

W. J. HERSCHEL

Littlemore, August 2

Meteors

ON August 4, 10h. 40m., a beautiful slow meteor was seen here threading its way from about 2° S. of ο Ursæ Majoris to very slightly below β Aurigæ. Its light fluctuated greatly, but at its best it must have been brighter than Jupiter, though the effect was much marred by mist. The most noteworthy feature was its extreme slowness of movement; a careful determination gave 8 seconds as the time it remained in sight. There was no train of any sort; the meteor rolled along with a star-like aspect, and its velocity near the end point became so much impeded that it seemed almost stationary. I observed fifty-seven other meteors during the same night, but none of these could be associated in appearance and direction with the one specially described. Its radiant-point was probably in Ursa Major, close to β, at about 162° + 59°.

On August 6, 10h. 3m., a meteor equal to Jupiter was seen pursuing a long path just south of and nearly parallel to α and ε Pegasi. It left a bright streak, and was a conspicuous object, notwithstanding the moonlight. The radiant-point was at about 32° + 17°, nearly 6° S.S.E. of α Arietis, or possibly in the extreme east boundaries of Aries.

It would be important to hear of duplicate observations of these large meteors. In the eastern parts of England they must have appeared very bright, and being visible at a convenient hour in the evening many persons will have noticed them.

Bristol, August 9 W. F. DENNING

LAST night at about eleven o'clock a fine meteor was visible here through an opening in clouds. Its path was in Aquarius, commenced a little to the east of η, and seemed to be in the direction of a line joining η and δ. In three or four seconds the meteor passed over about 20°, and it left momentarily a trail over the last 10°. This was slightly curved, the convex side being to the east, and the colour varying from yellow for a quarter of the curve to red during the remainder. At first the meteor resembled Saturn in size and colour, then became larger, whiter, and afterwards pale blue, and when it finally disappeared behind the clouds it considerably exceeded Venus at her brightest, both in size and brilliance.

L. J. H.

Ramsey, Isle of Man, August 5

PHYSIOLOGICAL SELECTION: AN ADDITIONAL SUGGESTION ON THE ORIGIN OF SPECIES[1]

II.

NEXT, let it be observed that we cannot expect to meet with much direct evidence of physiological selection from our domesticated varieties. For, first, breeders and horticulturists keep their strains separate artificially, and preserve many kinds of variation other than those of the reproductive system with which alone we are concerned; and, secondly, it is never the aim of these men to preserve this particular kind of variation. Therefore, all that we can here learn from our domesticated productions is the paramount importance of preventing intercrossing with parent forms, if a new varietal form is ever to gain a footing. No one of these domesticated varieties could have been what it now is unless such intercrossing had been systematically prevented by man; and this gives us good reason to infer that no natural species could have been what it now is unless every variety in which every species originated had been prevented from intercrossing with its parent form by nature. For the cases are extremely rare in which one species differs from another (living or extinct) in respect of any feature so highly utilitarian in character as to justify belief that the newer species owed its differentiation to natural selection having been able to overcome the swamping effects of free intercrossing.

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[1] Abstract of a Paper read before the Linnean Society on May 6, by George J. Romanes, M.A., LL.D., F.R.S. &c. Continued from p. 316.

Again, as to plants and animals in a state of nature, the particular variation with which we are concerned would scarcely be noticed until it had given rise to a new species. In this respect, therefore, the theory of physiological selection is in the same predicament as that of natural selection: in neither case are we able directly to observe the formation of one species out of another by the agency supposed; and, therefore, in both cases our belief in the agency supposed must to a large extent depend on the probability established by general considerations. Nevertheless, although our sources of direct evidence are thus seen to be necessarily limited, I shall now hope to show that they are sufficient to prove the only fact which they are required to prove—namely, that the particular kind of variation, on the occurrence of which my theory depends, does occur both in nature and under domestication.

One very obvious example of the particular variation which is required by the theory of physiological selection has already been given in the season of flowering or of pairing being either advanced or retarded. This I take to be a most important case for us, inasmuch as it is one that must frequently arise in nature. Depending as it chiefly does on external causes, numberless species both of plants and animals must, I believe, have been segregated by its influence. For in every case where a change of food, temperature, humidity, altitude, or of any of the other many and complex conditions which go to constitute environment—whether the change be due to migration of the species or to alterations going on in an area occupied by a stationary species—in every case where such a change either retards or promotes the season of propagation, there we have the kind of variation which is required for physiological selection. And it is needless to give detailed instances of such variation where this is due to so well known and so frequently observed a cause.

But it is on what may be called the spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection. The causes of variability are here much more numerous, subtle, and complex than are such extrinsic causes as those just mentioned; and they are always at work in the reproductive systems of all organisms. The consequence is, as Mr. Darwin has shown by abundant evidence, that variations in the direction of sterility depend more on what he calls the nature of the organism than on the influence of external conditions. Of this fact we have direct evidence, firstly in individuals, secondly in varieties, and thirdly in species.

(1) *Individuals.*—Mr. Darwin observes, "it is by no means rare to find certain males and females which will not breed together, though both are known to be perfectly fertile with other males and females. We have no reason to suppose that this is caused by these animals having been subjected to any change in their habits of life; therefore such cases are hardly related to our present subject. The cause apparently lies in an innate sexual incompatibility of the pair when matched." He then proceeds to give examples from horses, cattle, pigs, dogs, and pigeons, concluding with the remark that "these facts are worth recording, as they show, like so many previous facts, on what slight constitutional differences the fertility of an animal often depends." Elsewhere he gives references to similar facts in the case of plants; and instances of this individual incompatibility, both in plants and animals, might easily be multiplied.

Now, if even as between two individuals there may thus arise absolute sterility without there being in either of them the least impairment of fertility towards other individuals, much more may such incompatibility extend towards a number of individuals. For certainly the most remarkable feature about this individual incompatibility is the fact of its being only individual: it would not be nearly so remarkable, or antecedently improbable, if the incompatibility were to run through a whole race or strain. In the fact of individual incompatibility, therefore, we have the kind of variation which my theory requires, and this as arising spontaneously in the highest degree of efficiency.

(2) *Races.*—But of even more importance for us is the direct evidence of such a state of matters in the case of varieties, breeds, or strains. In the ninth chapter of the "Origin of Species," and in the nineteenth chapter of the "Variation of Plants and Animals under Domestication," Mr. Darwin adduces miscellaneous instances of varieties of the same species which exhibit a higher degree of fertility within themselves than they do with one another. In this respect, therefore, they resemble natural species; but as they are only domesticated varieties known to belong to the same species, they are here available as evidence of what may be termed racial incompatibility, or of the particular kind of variation which my theory requires. To quote only two instances: "The yellow and white varieties (of Verbascum) when crossed produce less seed than the similarly coloured varieties"; and the blue and red varieties of the pimpernel are absolutely sterile together, while each is perfectly fertile within itself. Such instances are the more suggestive on account of their arising under domestication, because, as a rule, domestication increases fertility, and is thus inimical to sterility—sometimes even breaking down the physiological barriers between natural species. Therefore, if in some cases even under domestication the reproductive system may vary in this manner, so as to erect physiological barriers between artificial varieties, much more are such barriers likely to be erected between varieties when these arise in a state of nature.

But as regards varieties in a state of nature, I have not been able to meet with any evidence of racial incompatibility. Nor is this to be wondered at: for, unless the degree of such incompatibility were well pronounced, it would not be noticed; while, if it were well pronounced, the two varieties would for this very reason be classified as species. Therefore, the fact of racial incompatibility within the limits of wild species could only be proved by experiments undertaken expressly for the purpose, in the way which I shall afterwards explain.

(3) *Species.*—According to the general theory of evolution, which in this paper is taken for granted, the distinction between varieties and species is only a distinction of degree; and the distinction is mainly, as well as most generally, that of mutual sterility. Therefore my theory of physiological selection is here furnished with an incalculable number of instances of the particular kind of variation which is required; for in so many instances as variation has led to any degree of sterility between parent and varietal forms—or between the varying descendants of the same form—in so many instances it is merely a statement of fact to say that physiological selection must have taken place. There remains, however, the question whether the particular change in the reproductive system which led to all these cases of mutual sterility was anterior or posterior to changes in other parts of the organism. For, if it was anterior, these other changes—even though they be adaptive changes—were presumably due to the sexual change having interposed its barrier to crossing with parent forms; while, if the sexual change were posterior to the others, the presumption would be that it was the latter which, by their reaction on the sexual system, induced the former. I shall have to consider this alternative later on. Here, therefore, it is enough to point out that under either possibility the principles of physiological selection must have been at work; only these principles are accredited with so much the more causal influence in the production of species in the proportion that we find reason to suppose the sexual change to have been, as a rule, the prior change. But under either alternative, and on the doctrine that species are extreme

varieties, we have many hundreds of thousands of instances of fertility within the varietal forms with sterility towards allied forms.

Probably enough has now been said to show that, as a matter of fact, the particular kind of variation in the reproductive system which is required by the theory of physiological selection does occur, firstly, in individuals; secondly, in races; and thirdly, in species. But the evidence of physiological selection as an agency in the evolution of species is so far only *primâ facie.* That is to say, although we have evidence to prove the occurrence of this particular kind of variation, and although we can see that whenever it does occur it must be preserved, as yet we have seen no evidence to show how far this kind of variation has been at work in the formation of species. I will, therefore, next proceed to give an outline sketch of the evidence which I have been able to find, tending to show that the facts of organic nature are such as they ought to be, if it is true that physiological selection has played any considerable part in their causation. And to do this I will begin by taking the three cardinal objections to the theory of natural selection with which I set out—namely, sterility, intercrossing, and inutility. For, as we shall see—and this in itself is a suggestive consideration—all the facts which here present formidable obstacles to the theory of natural selection, when this is regarded only as a theory of the origin of species, are not only explained by the theory of physiological selection, but furnish to that theory some of the best evidence which I have been able to find.

Argument from Sterility.—In what respects do species differ from one another? They differ firstly, chiefly, and most generally in respect of their reproductive systems: this, therefore, I will call the primary difference. Next, they differ in an endless variety of more or less minute details of structure, which are sometimes adaptive and sometimes not. These, therefore, I will call the secondary differences. Now, the secondary differences are never numerous as between any two allied species: in almost all cases they admit of being represented by units. Yet, if it were possible to enumerate all the specific differences throughout both the vegetable and animal kingdoms, there would be required a row of figures expressive of many millions. In other words, the secondary specific distinctions may occur in any parts of organisms, but never occur in many parts of the same organism. So that, if we have regard to the whole range of species, the secondary distinctions are seen to be, in the highest imaginable degree, variable or inconstant: the only distinction which is at all constant or general is the primary distinction, or the one which belongs exclusively to the reproductive system. Surely, therefore, what we primarily require in any theory of the origin of *species*, is an explanation of this relatively constant or general distinction. But this is just what all previous theories fail to supply. Natural selection accounts for some among the many secondary distinctions, but is confessedly unable to explain the primary distinction. The same remark applies to sexual selection, use and disuse, economy of growth, correlated variability, and so forth. Even the prevention of intercrossing by geographical barriers or by migration is unable to explain the very general occurrence of some degree of sterility between two allied varieties which have diverged sufficiently to take rank as different species. All these theories, therefore, are here in the same predicament: they profess to be theories of the origin of species, and yet none of them is able to explain the one fact which more than any other goes to constitute the distinction between species and species. The consequence is that most evolutionists here fall back upon a great assumption: they say it must be the change of organisation which causes the sterility—it must be the secondary distinctions which determine the primary. But the contrary proposition is surely at least as probable, namely, that it is the sterility which, by preventing intercrossing with parent forms, has determined the secondary distinctions—or, rather, that it has been the original condition to the operation of the modifying causes in all cases where free intercrossing has not been otherwise prevented. For, obviously, it is a pure assumption to say that the secondary differences have always been historically prior to the primary difference, and that they stand to it in the relation of cause to effect. Moreover, the assumption does not stand the test of examination, as I will now proceed to show.

(1) On merely *a priori* grounds it scarcely seems probable that whenever any part of any organism is slightly changed in any way by natural selection, or by any other cause, the reproductive system should forthwith respond to that change by becoming sterile with allied forms. Yet this is really what the assumption in question requires, seeing that *all* parts of organisms are subject to the secondary specific distinctions. What we find in nature is a more or less constant association between the one primary distinction and an endless profusion of secondary distinctions. Now, if this association had been between the primary distinction and some one—or even some few—secondary distinctions, constantly the same in kind; in this case I could have seen that the question would have been an open one as to which was the conditional and which the conditioned. But as the case actually stands, on merely antecedent grounds, it does not appear to me that the question is an open one. Here we have a constant peculiarity of the reproductive system, repeated over and over again—millions of times—throughout organic nature; and we perpetually find that when this peculiar condition of the reproductive system is present, it is associated with structural changes elsewhere, which, however, may affect any part of any organism, and this in any degree. Now, I ask, is it a reasonable view that the one constant peculiarity is always the result, and never the condition, of any among these millions of inconstant and organically minute changes with which it is found associated?

(2) But, quitting *a priori* grounds, it is a matter of notorious fact that in the case of nearly all our innumerable artificial productions, organisms do admit of being profoundly changed in a great variety of ways, without any reaction on the reproductive system following as a consequence.

(3) Again, as regards wild species, Mr. Darwin proves that "the correspondence between systematic affinity and the facility of crossing is by no means strict. A multitude of cases could be given of very closely allied species which will not unite, or only with extreme difficulty; and, on the other hand, of very distinct species which unite with the utmost facility." And he goes on to say that "within the limits of the same family, or even of the same genus, these opposite cases may occur"; so that "the capacity of the species to cross is often completely independent of this systematic affinity, that is, of any difference in their structure or constitution, excepting in their reproductive systems." Now, on the supposition that sterility between species is always, or generally, caused by the indirect influence on the reproductive system of changes taking place in other parts of the organism, these facts are unintelligible—being, indeed, as a mere matter of logic, contradictory of the supposition.

(4) Mr. Darwin further shows that, "independently of the question of fertility, in all other respects there is the closest general resemblance between hybrids and mongrels." Clearly, this fact implies that natural selection and artificial selection run perfectly parallel in all other respects, save in the one respect of reacting on the reproductive system, where, according to the views against which I am arguing, they must be regarded as differing, not only constantly, but also profoundly.

(5) Lastly, Mr. Darwin concedes—or rather insists—

that "the primary cause of the sterility of crossed species is confined to differences in their sexual elements." A general fact which assuredly proves that the primary specific distinction is one with which the organism as a whole is not concerned: it is merely a local variation which is concerned only with the sexual system. Why, then, should we suppose that it differs from a local variation taking place in any other part of the organism? Why should we suppose that, unlike all other such variations, it can never be independent, but must always be superinduced as a secondary result of changes taking place elsewhere? It appears to me that the only reason why evolutionists suppose this is because the particular variation in question happens to have as its result the origination of species; and that, being already committed to a belief in natural selection or other agencies as the causes of such origination, they are led to regard this particular kind of local variation as not independent, but superinduced as a secondary result of these other agencies operating on other parts of the organism. But once let evolutionists clearly perceive that natural selection is concerned with the origin of species only in so far as it is concerned with the origin of adaptive structures—or of some among the secondary distinctions—and they will perceive that the primary specific distinction takes its place beside all other variations as a variation of a local character, which may, indeed, at times be due to the indirect influence of natural selection, use, disuse, and so forth; but which may also be due to any of the numberless and hidden causes that are concerned with variation in general.[1]

I trust, then, that reasons enough have now been given to justify my view that, if we take a broad survey of all the facts bearing on the question, it becomes almost impossible to doubt that the primary specific distinction is, as a general rule, the primordial distinction. I say "as a general rule," because the next point which I wish to present is that it constitutes no part of my argument to deny that in some—and possibly in many—cases the primary distinction may have been superinduced by the secondary distinctions. Indeed, looking to the occasional appearance of partial sterility between our domesticated productions, as well as to the universally high degree of it between genera, and its universally absolute degree between families, orders, and classes, I see the best of reasons to conclude that in *some* cases the sterility between *species* may have been originally caused, *and in a much greater number of cases subsequently intensified*, by changes going on in other parts of the organism. Moreover, I doubt not that of the agencies determining such changes natural selection is probably one of the most important. But what does this amount to? It amounts to nothing more than a re-statement of the theory of physiological selection. It merely suggests hypothetically the cause, or causes, of that particular variation in the reproductive system with which alone the theory of physiological selection is concerned, and which, as a matter of fact, *howsoever caused*, is found to constitute the one cardinal distinction between species and species. Therefore I am really not concerned with what I deem the impossible task of showing how far, or how often, natural selection—or any other cause—may have induced this particular kind of variation in the reproductive system by its operations on other parts of an organism. Even if I were to go the full length that other evolutionists have gone, and regard this primary specific distinction as in all cases due to the secondary specific distinctions, still I should not be vacating my theory of physiological selection: I should merely be limiting the possibilities of variation within the reproductive system in what I now consider a wholly unjustifiable manner. For, as previously stated, it appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number.[1]

If we thus regard sterility between species as the result of what I have called a local variation, or a variation arising only in the reproductive system—whether this be induced by changes taking place in other parts of the organism, to changes in the conditions of life, or to changes inherent in the reproductive system itself—we can understand why such sterility rarely, though sometimes, occurs in our domesticated productions; why it so generally occurs in some degree between species; and why as between species it occurs in all degrees.

It rarely occurs in our domesticated productions because it has never been the object of breeders or horticulturists to preserve this kind of variation. Yet it sometimes does occur in some degree among our domesticated productions, because the changes produced on other parts of the organism by artificial selection do, in a small percentage of cases, react upon the reproductive system in the way of tending to induce sterility with the parent form, while not lessening fertility with the varietal form. Again, this particular condition of the reproductive system is so generally characteristic of species simply because in as many cases as it occurs it has constituted the reason why species exist as species. And, lastly, this particular variation in the reproductive system has taken place under nature in such a variety of degrees—from absolute sterility between species up to complete, or even to more than complete, fertility—because natural species, while being records of this particular *kind* of variation are likewise the records of all *degrees* of such variation which have proved sufficient to prevent overwhelming intercrossing with parent forms. Sometimes this degree has been less than at other times, because other conditions—climatic, geographical, habitatorial, physiological, and even psychological[2]—have co-operated to prevent intercrossing, with the result of a correspondingly less degree of sterility being required to secure a differentiation of specific type. Lastly, where species have been evolved on different geographical areas, or by use, disuse, and other causes of a similarly "direct" kind, there has been no need to prevent intercrossing in any degree; so that allied species formed under any of these conditions may still remain perfectly fertile, or even more than naturally fertile, with one another.

In view of these considerations, I should regard it as a serious objection to my theory if it could be shown that sterility between allied species is invariably absolute, or even if it could be shown that there are no cases of unimpaired fertility. What my theory would expect to find is exactly what we do find—namely, an enormous majority of instances where sterility occurs in all degrees, with a few exceptional instances where secondary distinctions have been able to develop without being associated with the primary distinction. So that, on the whole, I cannot but candidly consider that all the facts relating to the sterility of natural species are just what they ought to be, if they have been in chief part due to the principle which I am advocating. Mr. Darwin appears to have clearly perceived that there must be some one principle serving to explain all these facts—so curiously related, and yet so curiously diverse. For he says, and he says most truly, "We have conclusive evidence that the sterility of species must be due to some principle quite independent of natural selection." I trust I have now said enough to show that, in all probability, this hitherto undetected principle is the principle of physiological selection.

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[1] Mr. Darwin himself does not appear to have held the view against which I am now arguing—viz. that the primary distinction is always, or usually, superinduced by the secondary. Not even here, therefore, is his authority opposed to my views: upon this question his voice is merely silent.

[1] The paper here develops another line of argument which it is difficult to render in abstract. Its object, however, is to show that, even in the cases where the primary distinction is superinduced by the secondary—whether these cases are, as I believe, "comparatively few" or comparatively numerous—my theory is available to explain why the primary distinction is so habitual an accompaniment of the secondary distinctions, of whatever kinds or degrees the latter may happen to be. For, according to my theory, the reason of this association in such cases is that it can only be those kinds and degrees of secondary distinction which are able so to react on the reproductive system as to induce the primary distinction that are, *for this reason*, preserved. Or, otherwise expressed, in cases where the secondary distinctions induce the primary, the former owe their existence to the fact that they happened to be of a kind capable of producing this particular effect. Under this view, even in these cases it is the principles of physiological selection that have determined the kinds of secondary distinction which are allowed to survive. For these principles have, in all such cases, *selected* the particular kinds of secondary distinction which have proved themselves capable of so reacting on the reproductive system as to bring about the primary distinction—a general view of the subject which appears to be justified by the very general association between the two.

[2] See "Origin of Species," p. 81, where it is shown that among vertebrated animals different varieties of the same species, even when living on the same area, frequently exhibit a marked repugnance to pairing with one another. In the same passage, it is remarked the different varieties sometimes occupy different stations.

(*To be continued.*)

RED SUNSETS AND VOLCANIC ERUPTIONS

THE great volcanic eruption in New Zealand raises anew the question of the connection between volcanic eruptions and sunset phenomena arising from attenuated matter in the upper regions of the atmosphere. The theory that the noteworthy sunsets about the end of 1883 were due to the Krakatao eruption has been questioned on the ground that, in many parts of the world, these red sunsets have continued until the present time, though not with the same intensity as in 1883. Beautifully variegated sunsets have always been very common in this country. The result was that the sunset phenomena of 1883 did not appear to us as anything new in kind, but only as an intensification of something with which we were already familiar. In order to reach a decisive conclusion we must have observations made in regions where the upper atmosphere is exceptionally free from vapours or other attenuated matter. The advent of such matter can then be detected when it could not be detected at other places. Among such regions I would suggest South Africa, especially the Cape of Good Hope. During my brief residence there in November and December of 1882, nothing was more striking than the extreme whiteness and purity of the western twilight. If such a twilight is there the rule during the whole year, then any diffusion of volcanic vapour in the upper atmosphere must produce a very striking effect. I would therefore suggest to observers in that region the value of precise information on this point, especially with a view of learning to what extent, and within what time, the red sunsets of 1883 disappeared, and whether any such phenomena now reappear as the result of the volcanic eruption in New Zealand. S. NEWCOMB

MR. FORBES'S EXPEDITION TO NEW GUINEA

NATURALISTS will be glad to learn that a collection of natural history objects has been sent to England by our countryman Mr. H. O. Forbes, who has been doing good work on the Astrolabe Mountains in south-eastern New Guinea. Unfortunately a lack of proper support appears to have greatly crippled the efforts of this energetic traveller, who fears that he may be compelled to abandon his proposed expedition to Mount Owen Stanley, the ascent of which was the primary object of his explorations on leaving England. The disaster which befel Mr. Forbes at Batavia, where the boat with all his equipment for the expedition was capsized and everything lost, will be fresh in the recollection of our readers (see NATURE, vol. xxxii. p. 552), and it is only by the utmost display of courage and energy, and by a large pecuniary sacrifice on his own part, that Mr. Forbes has been able to fit out an expedition to New Guinea from Brisbane. It is to be hoped that the great Societies of this country and Australia will not allow this expedition to come to an end for lack of funds. Mr. Forbes has shown what he can do in the cause of science, and a little timely help would now enable him to conduct the work of exploration on which his soul is bent and bring it to a successful issue. It is not generally known in this country that during his last expedition to the Malay Archipelago he expended more than 600*l.* of his own money in endeavouring to render his expedition more complete, and nothing but a little generous encouragement is needed to enable him to sustain the serious pecuniary loss which has befallen him in his attempt to reach Mount Owen Stanley. At the time of writing, we hear that there is a prospect of Australia coming to the rescue and aiding Mr. Forbes towards the attainment of his object, and we trust that England will also do something for a man who, as an explorer and a naturalist, has reflected credit on his country.

The district recently explored by Mr. Forbes has been visited before by Mr. Goldie and Mr. Hunstein, the latter of whom has procured some remarkable novelties among the Birds of Paradise, which have been recently described by Dr. Finsch and Dr. Meyer (*Zeitsch. Ges. Orn.* ii. pp. 369-391). Hunstein has indeed penetrated further than Mr. Forbes was able to do on the present occasion, as the latter has as yet only worked the Sogeri district from a height of 1000 to 5000 feet, and this only in the rainy season.

Among the many interesting species found by Mr. Forbes in the Sogeri district may be mentioned *Harpyopsis novæ-guineæ*, Salvad, *Charmosyna stellæ*, Meyer, *Psittacella pallida*, Meyer (scarcely to be distinguished from *P. brehmi* of Mount Arfak), *P. madaraszi*, Meyer, *Eos incondita*, Meyer, *Parotia lawesi*, Ramsay, *Lophorina minor*, Ramsay, *Paradisornis rudolphi*, Meyer, *Amblyornis subalaris*, Sharpe, *Paradisea raggiana*, Sclater, *Microdynamis parra*, Salvad (*Rhamphomantis rollesi*, Ramsay), *Melidectes emilii*, Meyer, *Rallicula rubra*, Schlegel, and many other notable species, amongst which are two which appear to be undescribed, viz.,

Melirrhophetes batesi, sp. n.

M. similis *M. ochromelani*, Meyer, sed fasciâ supraoculari cervina distinguendus. Long. tot. 9·3, culm. 1·35, alæ 4·8, caudæ 4·2, tarsi 1·15.

Pseudogerygone cinereiceps, sp. n.

P. similis *P. flavilaterali*, Gray, ex Novâ Caledoniâ, sed minor, et rectricibus haud subterminaliter albo-fasciatis distinguenda. Long. tot. 3·4, culm. 0·4, alæ 1·9, caudæ 1·2, tarsi 0·65.

It was a little unfortunate for Mr. Forbes that Mr. Hunstein should have visited the Horse-shoe Range so shortly before the arrival of the former in New Guinea, but it says much for the complete way in which Mr. Forbes does his work of exploration, that he should have obtained specimens of nearly every one of the new species discovered by Mr. Hunstein. Unfortunately the Birds of Paradise were out of colour at the time of his visit, but the specimens sent are of great interest, as showing the moults and changes of plumage in these birds. Of the extraordinary species known as Prince Rudolph's Bird of Paradise (*Paradisornis rudolphi*) with blue wings and blue flank-plumes, Mr. Forbes secured only one, apparently a female. As these surprising novelties have been discovered in the Astrolabe Range, which has an elevation of less than 5000 feet, what prizes and discoveries may not be awaiting the explorer if he reaches Mount Owen Stanley with an altitude of 13,000 feet? May he succeed!

R. BOWDLER SHARPE

THE PERSISTENT LOW TEMPERATURE

IT is seldom that the weather maintains such a decided persistency for temperatures below the average conditions. Week after week passes, and the thermometrical